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ETHICS AND THE BEETLE

By A. M. MACIVER

IF I tread wantonly on a woodlouse, I do wrong. It is an evasion to pretend that the act is in itself morally indifferent and then say that it ought nevertheless to be condemned as gratification of an impulse of cruelty which would have produced wrongdoing if gratified upon a human being. There would be nothing wrong about the impulse, as gratified in this case, unless this act were, in and by itself, wrong. But it is only a very small wrong, and to exaggerate its wrongfulness is sentimentality. The Jain who hangs a veil in front of his mouth for the protection of the small flies who might otherwise fly into it is to be condemned—but only for lack of sense of proportion. To devote so much attention to avoiding committing such infinitesimal offences is inevitably to divert attention which might otherwise have been given to avoiding offences vastly more serious. Little wrongs have to be done, in order that greater wrongs may be avoided. If I kill a Colorado beetle, I do wrong by the beetle ; but, if I fail to kill it, I do wrong by all the growers and consumers of potatoes, and their interests are vastly more important.

I may be told that this doctrine is dangerous in its implications. And so it *is*. Any doctrine is dangerous which allows that considerations of the agent's own advantage can ever justify an action in and by itself wrong—as I assume whenever I kill a mosquito because otherwise it might bite me. For the applications of the principle involved extend far beyond stepping on beetles or swatting mosquitoes. General acceptance of it would not even—as might perhaps be thought—encourage kindness to animals. Extreme kindness to animals results from a largely sentimental extension to animals of some of the larger or more familiar species—perhaps all those large enough, or familiar enough, for their sufferings, when they suffer, to be visible to human eyes—of our normal feelings for our fellow human beings. If it is once observed that there is no difference in principle between the case of dogs, cats or horses, or stags, foxes and hares, and that of tsetse-flies or tapeworms or the bacteria in our own blood-stream, the conclusion likely to be drawn is that there is so much wrong that we cannot help doing to the brute creation that it is best not to trouble ourselves about it any more at all. The ultimate sufferers are likely to be our fellow

men, because the final conclusion is likely to be, not that we ought to treat the brutes like human beings, but that there is no good reason why we should not treat human beings like brutes. Extension of this principle leads straight to Belsen and Buchenwald, Dachau and Auschwitz, where the German and the Jew or Pole only took the place of the human being and the Colorado beetle.

But no principle is proved *false* by the observation that it is likely to be abused, if generally known. As soon as thought is required to settle a moral problem, some people will be tempted to sophistry to reach a conclusion agreeable to their own vicious inclinations. But anyone who thinks must see that moral choice is hardly ever a straight choice between right and wrong, but normally only a choice between the lesser and the greater wrong —that it always involves balancing right against right and wrong against wrong. It would be absurd to argue that, if a balance must be struck, it does not matter how or where it is struck : on the contrary, that is *all* that matters, being just what is difficult. From the fact that I am entitled to kill a mosquito to save myself from the inconvenience of being bitten it in no way follows that the authorities of the German Reich were entitled to kill Jews in order to relieve Germany of the inconvenience of harbouring a partially unassimilable Jewish community. Yet this is not because the principle involved is different ; the principle is *the same* ; similar considerations have to be balanced against one another ; only, in the one case (we feel sure) the balance has been struck right—in the other it has been struck wrong.

I feel no doubt in my own mind that this general principle is sound. It represents a deliverance of my "moral consciousness" quite as clear as any other. I can see nothing in the points of difference between the human and other animal species to explain why the question whether or not a creature is capable of suffering *wrong* should be held to depend upon its zoological classification. Even if morality is held to be a merely social institution, though the consequence may follow that moral obligations only exist towards fellow members of the same society, it does not follow that they can only exist towards human beings. Fellow members of a society are those, whoever they may be, who are *regarded* in the society as fellow members by the others. I myself feel that the cat and the dog who live in the same house with me are fellow members of my family circle, closer related to me socially than the human neighbour next door whom I know only by sight and name, and infinitely closer than some odd Brazilian or Melanesian, with whom my only connection is that we are fellow men.

Take it as agreed, then, that animals can have wrong—real moral wrong—done them. Can they also themselves *do* wrong? Certainly we do not often *condemn* them as having done wrong, except as a result of confusion of thought. We speak of " punishing " a dog for a fault—we say that a puppy or a kitten has been " naughty "—but, if we reflect, we see that we are then either speaking metaphorically or else sentimentally treating the dog or cat as if it were a human being, with human understanding and reasoning powers, which it is not. Bradley is quite right in pointing out that the so-called " punishment " of a dog is not real punishment in the moral sense, but only " correction "—what we have now learnt from Pavlov to call " conditioning ". If there is to be real *moral* condemnation of even cats or dogs—not to speak of frogs, codfish or centipedes—it can only be for extremely simple and primitive moral failings. But what about a cat (for example) which is a " bad mother "? How does her case differ from that of a human mother acting similarly?

We must distinguish two levels of judgment applied in the case of the human mother, only one of which can possibly be applicable to the cat. We hold that even a mother who lacks natural affection for her child ought to recognise that it is her moral duty to treat it kindly, and condemn her if she neglects that duty. This is clearly a duty which exists only for a rational being capable of reflection, and therefore not for the cat. But such a human mother, however conscientiously she did her duty by the child without natural affection, would still be condemned by some (and indeed, I think, by most) of us for the want of affection itself. Not, indeed, by those who hold fast through thick and thin to the principle that " nobody can be blamed for what he can't help "—but are they not allowing a philosophical theory to distort what would be their unsophisticated moral judgment? Perhaps we ought to say that there are two kind of moral condemnation, of which only that which applies to deliberate actions is normally understood by " blame ". Exclusive attention to this one kind of moral condemnation goes with a conception of morality as concerned only with the deliberate actions of human beings—and perhaps even only of human beings of comparatively high intelligence. But we do not in ordinary life confine moral condemnation within these limits. One who is wholly without noble impulses we reckon a " bad man ", without asking whether it is in any way the result of his own doing that he lacks them. Is not this also moral condemnation? In this sense the cat who neglects her kittens might also be (though not " blamed ", yet) morally condemned.

And we should do well not to overlook the parallel case of human children. Nobody who has ever watched two or more

dogs playing together can fail to recognise the similarity to the behaviour of young children. The children, unlike the animals, will ultimately grow up into adult human beings, but does all application of moral judgments to children actually refer forwards to this future development? Surely the "naughty" child has, at least in some cases, actually done wrong—not merely done something, in itself morally indifferent, which needs to be "corrected" only because it might establish a habit which would create a standing temptation to wrong action in the adult.

If this is disputed, then we may ask: when does irresponsible childhood end and adult responsibility begin? Legal majority may be reached in an instant, on the first stroke of twelve on the morning of the twenty-first birthday, but moral majority can only be attained gradually and imperceptibly. Some people—intelligent people, not idiots or lunatics—remain childish in their behaviour all their lives. Do moral judgments not apply to them on that account? It cannot be said that they ought to have made themselves moral agents and are therefore to blame for their failure to do so, because, by hypothesis, no "ought" applies except to those who are moral agents already. In fact we bestow both moral condemnation and commendation on such people, taking them as they are. We condemn their childishness and irresponsibility as moral failings, thought not failings for which they are "to blame". We commend (perhaps) their kindliness, their considerateness, their patience, their industry, their cheerfulness in misfortune, just as we might commend the same qualities if we found them in children. Most of the moral judgments that we pass every day are (I suspect) judgments of this kind. That is to say, the morality which we apply every day is not the morality of the moral philosophers, with its stress on freedom and responsibility and the deliberate acts of rational agents, but a matter of approval and disapproval of moral characters, revealed perhaps in impulsive actions, often by very simple souls. In this sense human beings could still show morally good and bad characters, even if they were intellectually on the level of the brutes, though the moral standards applicable would certainly be very different from those applicable to normally intelligent human beings.

But this seems to cut away all grounds for supposing that the characters of the brutes themselves cannot be morally good or bad. As we saw before that the wrong which can be done to a beetle is a very insignificant wrong, though still wrong, so the moral virtue which can exist in a beetle, distinguishing it from other less virtuous beetles, is likely to be a very slight sort of virtue, but still virtue. We cannot ourselves

detect it, because we know so little about the intimate lives and individual characters of beetles that for almost all practical purposes all beetles of the same species are to us alike. But we have no reason to deny that, though imperceptible to us, it must be there.

This conclusion is contrary to the whole tradition of moral philosophy. Why have others in the past not seen what seems now (to me at least) so obvious? I believe that the explanation is something which I have already had occasion to point out elsewhere.[1] The ethical tradition became established in a state of human society which threw great stress on *responsibility*, but recent social developments have been such as largely to eliminate responsibility, as our fathers and grandfathers knew it. But moral philosophy has not yet become adjusted to this fact.

A good enough example of the traditional attitude is found in a characteristically violent footnote in Bradley's *Ethical Studies*:[2]

> If many persons meant what they said, animals are moral and responsible, and animals are punished. And a time would seem coming, when we shall hear of 'the rights of the beast'. Why *not*, in Heaven's name? Why is the beast not a subject of right, civil at least, if not political? But this is for our emancipators of the future. We are content to hold the vulgar creed that a beast is no moral agent, actual or possible; is not responsible, nor the subject of rights, however much the object of duties.

What is to be observed here is the tacit assumption that being a "moral agent", being "responsible", and being a "subject of rights", go together. To Bradley's contemporaries this apparently seemed self-evident, but it may not seem so to us.

Bradley does not merely deny that animals have rights. He implies that they cannot have rights *because* they are not "responsible" and not "moral agents". I cannot make out what lies behind this, if it is not a faint echo of the prehistoric legal theory of voluntary subjection to the jurisdiction of the courts. The courts will only listen to a claim if made by one against whom a similar claim might be made, because it is felt to be unfair to expect anyone to make the sacrifice of submitting to the jurisdiction, except at the instance of one who has made an equal sacrifice. The ethical tradition has grown up against a background of legal practice. In prehistoric courts of law heads of households—each in his own household exercising authority

[1] *Aristotelian Society Proceedings*, 1945–46, pp. 188–90.

[2] Edition of 1927, pp. 31–2.

over his dependent wife and children and his living chattels, whether human (slaves) or non-human (domestic animals)—met solemnly as equals to settle their disputes, since these could not be settled by authority, according to agreed rules of right and wrong. Subjection to law was thus a privilege going with responsibility: the bulk of mankind, together with the animals, were not under law, but under authority. And the status of a " moral agent " in the ethical tradition still remains recognisably the privileged status of the primitive head of a household, though all adult human beings have now been promoted to the dignity of " responsibility ", leaving only children temporarily and animals permanently outside. Morality still concerns only those who have responsibility.

This was not felt as an arbitrary limitation so long as most members at least of the social classes within which works of moral philosophy were written and read actually felt upon themselves a daily and hourly burden of real personal responsibility. The moral difficulties which engrossed attention were those of " responsible men ", who had to make decisions which might have vital consequences. But now we are all coming " under authority " again—though not now the old personal authority of the master, but the impersonal authority of the social machine as a whole. More may be " free " in the sense that (through reduction in the hours of work and so forth) there are larger spheres and periods in which they can do what they like, but far fewer (and fewer every year) experience *responsibility*, in the sense that much less that is significant can be seen to depend upon any decision of any individual. In consequence our ordinary everyday morality has come to be much more like that of the *slaves* in older societies than like that of the *masters*. The difficulties of " responsible men " mean little or nothing to us. We judge, not " free decisions ", but moral characters, regarding intentions more than achievements, knowing by experience that the issue of individual actions is largely beyond individual control.

It is when we go forward from this actual morality which we apply every day that, looking again at our humbler fellow citizens of the universe—beetles and woodlice as well as cats and dogs—we may decide that their status as moral beings differs from our own, after all, only in degree. (Of course I do not wish to deny that this difference of degree is immense; but we ought not to misrepresent its nature in our philosophy).

University of Leeds, February, 1948.

MR. URMSON ON THE WORD "PROBABLE"

By H. WALLIS CHAPMAN

THE distinction which Mr. Urmson has pointed out between two senses of the word "probable"[1] is interesting and important, but I do not think that he has been equally successful with the details, and I wish to consider it further.

Keynes[2] has hinted at such a distinction more than once, as when he contrasts "induction and analogy" with "statistical frequency" (p. 109), but he has not worked it out. He twice (pp. 222, 291) speaks of propositional functions rather than propositions, but does not follow this up. On p. 291 he speaks of the probability

$$\phi(x) / S(x) \cdot h$$

where x is a variable and $\phi(x)$ and $S(x)$ propositional functions. But this does not agree with his definition of the probability relation on p. 4 where he speaks of it as holding between *propositions*.

We are thus led to two types of probability as distinguished by Mr. Urmson, the probability that any x satisfying $S(x)$ satisfies $\phi(x)$, which must be written '$\phi(x) / S(x) \cdot h$', and the probability that every $S(x)$ satisfies $\phi(x)$, or

$$\{(x) \cdot S(x) \supset \phi(x)\}/h .$$

'h' will in each case be used, as it is by Keynes, to denote a proposition, or conjunction of propositions, assumed as known. In practice this will contain the more or less analysed mass of propositions which we call common knowledge, and it may be important to remember this when an appeal is made to experience or intuition.

These two expressions are obviously of different forms, and the probabilities differ accordingly. They are both equal when either is equal to 1, i.e. they are then both certainties, but I cannot think of any other case, except that in which the class defined by $S(x)$ has only one member; this will be considered later. Otherwise the first probability is greater than the second. We cannot even say that evidence favourable to the one is favourable to the other; evidence showing that some rule *usually* holds may also make it more probable, or even certain, that exceptions exist.

There are many other interesting questions as to the relations between the two forms, but I will now proceed to apply this notation to Mr. Urmson's four probability statements on p. 12.

[1] *Analysis* 8, 1, Oct., 1947.

[2] *A Treatise on Probability*, 1921.

"The probability of a European understanding the word 'waistcoat'" must be written.

$$\frac{\text{'}(x \text{ understands the word waistcoat})}{(x \text{ is a European}) \,.\, h\text{'}}$$

We will call this probability 'p'.

"The probability of a European understanding the word 'waistcoat' if he is an adult Englishman" is

$$\frac{\text{'}(x \text{ understands the word 'waistcoat'})}{(x \text{ is a European}) \,.\, (x \text{ is an adult Englishman}) \,.\, h\text{'}}$$

which we will call 'q'.

Then Mr. Urmson's first statement, viz. "The probability of a European understanding the word 'waistcoat' is very high relative to the evidence that he is an adult Englishman" seems capable of three meanings:

(a) q is very nearly unity;

(b) q is considerably greater than p;

(c) p is very little less (of course it may be greater) than the probability

$$\frac{(x \text{ is an adult Englishman})}{(x \text{ is a European}) \,.\, h}.$$

These three meanings are closely connected, especially (a) and (c), but they are distinguishable. I have little doubt that (a) represents Mr. Urmson's meaning, but I think the possibility of the other two has confused him.

The second probability appears as '$(x$ understands the word 'waistcoat')/$(x$ is an adult European$)$. $(x$ is an Englishman$)$. h'. This is clearly the same as the probability q of the first statement, the supposal in each case being

'$(x$ is a European$)$. $(x$ is an adult$)$. $(x$ is an Englishman$)$' and the same remarks apply.

The third probability is

$$\frac{(x \text{ understands the word 'waistcoat'})}{(x \text{ is adult}) \,.\, (x \text{ is an Englishman}) \,.\, h}.$$

Now this is not formally equivalent to the preceding probability q. They are equivalent materially if we allow that

$$(x) \,.\, (x \text{ is an Englishman}) \supset (x \text{ is a European})$$

which may be considered to be included in h. But this is not explicitly stated and might be denied, e.g. if it were held that a child of English parents born and brought up outside Europe is English but not European. In this case we could not assert that the probabilities were equal. This implication is therefore evidence to which the probability in question is relative, but it is not specificatory evidence.

The fourth probability is

$$\frac{(x \text{ understands the word 'waistcoat'})}{(x \text{ is an adult Englishman}) \,.\, h}$$

which is clearly the same as the third.

All the statements assert absolute probabilities if taken in the manner I have denoted by (*a*), and Mr. Urmson's distinction between the fourth and the other three arises from the fact that it is definite while they are ambiguous.

We also notice that the words 'Englishman', 'European' and 'adult' always occur in the same manner, and no one denotes 'specificatory evidence' more than another. Generally any name applied to the subject of a probability of type one affords specificatory evidence. Mr. Urmson's example (p. 13) of the European living to be a hundred years old is invalid, because the word 'European' provides specificatory evidence. The truer, and I think more natural, form of the objection suggested would be a request for *further*[3] evidence, which the replies suggested do not provide. I think, however, that there is an important truth at the bottom of Mr. Urmson's argument, and it is that, unless a statement as to the value of any probability is taken as intuitive, the grounds for it must contain a statement as to the value of some probability (or frequency, if these are to be distinguished).

I should formulate the distinction between specificatory and non-specificatory evidence as follows: specificatory evidence occurs as a propositional function, non-specificatory as a proposition. As the supposal in a probability of the second type is a proposition, such a probability is not relative to specificatory evidence in the ordinary sense. I hope to show, however, that it is or may be relative to propositional functions in which the ϕ or S, or parts of them, occur as variables.

This brings us to Mr. Urmson's third and most important distinction. He asks, "Is it probable that all cows are ruminant on the evidence that all examined cows have been ruminant?" and denies that this is an empirical question on the ground that no light is thrown on it by examining more cows. But light is thrown on it by examining more questions. Not, of course, if the question is considered merely as an example of the general problem of induction, "Is it probable that, if things exhibiting the property A have been found in all examined cases to exhibit the property B this conjunction will hold in cases of the property A as yet unexamined?" I have no solution of this hoary problem, but must point out that it applies equally to both types of probability; without it we have no reason to suppose that

[3] There is a difficulty here; the probability on further evidence would be a different probability, but I cannot go into this at present.

statistics of examined cases will give any clue to frequencies in unexamined ones. If, however, we assume this we can use it as a basis for a theory of probabilities. If, for example, before the discovery of Australia, it had been asked " Is it probable that all swans are white on the evidence that all swans hitherto examined have been white ? " a reasonable answer would have been, " Yes, but not so probable as that all cows ruminate, as we have many more instances of resemblances of the kind denoted by the use of such words as ' swan ' or ' cow ' being accompanied by differences in colour than by complicated differences in the digestive process : black cats and white cats are equally cats, but we have nothing to correspond to the case of the non-ruminating cow." The whole science of taxonomy deals with questions of this sort.

It follows that a probability statement of the second type may be an empirical statement to be tested by experience (I do not offer this as a complete account of statements of either type), but the empirical evidence is of a different kind from that appropriate to statements of the first type, it relates to generalizations not to instances. As generalizations are much harder to classify accurately than instances we can see why numerical evaluation of these probabilities is generally impracticable. A probability statement of the second type does not, however, necessarily involve empirical generalization, and it is easy to devise trivial instances in which probabilities of this type have definite numerical values. For example let it be known that one of two bags contains white balls only, in the other half the balls are black and half are white ; a bag is chosen and a ball is drawn and found to be white, what is the probability that every ball in the bag chosen is white ? The answer is 2/3 ; the corresponding probability of the first type is the probability that any ball (i.e. a ball drawn at random) in the bag chosen will be white and is 5/6. (For a proof see Appendix).

Before dealing further with the relation of the two types, I will consider a form of probability statement which is not obviously included in either. I refer to statements about individuals. Such probabilities would at first sight be denoted by ' $\phi(a)/h$ ', where a is a constant. But do such probabilities always exist ? Keynes' language suggests that he thought they did, but I find it very hard to believe that a probability-relation exists between a statement about a particular subject and evidence which makes no reference, direct or indirect, to that subject. In other words, it is a necessary condition for the existence of a probability $\phi(a)/h$ that h should contain or entail some proposition $S(a)$.

We can therefore write our probability as '$\phi(a)/S(a) . h$'.

Keynes points out (p. 58) that this probability cannot be simply obtained from the probability of the first type $\phi(x)/S(x) \,.\, b$ by means of the applicative principle, and if a is a genuine proper name I do not see how the probability could be dealt with, but I do not think that such cases really occur. This corresponds to Venn's view that probability is a property of series only, but it does not imply his conclusion that all probabilities are frequencies. Usually, at any rate, a is a disguised description, or $x=a \,.\equiv.\, a(x)$ where $a(x)$ is some propositional function.

Our probability can then be written '$\phi(x)/S(x) \,.\, a(x) \,.\, b$', which is of type one.

We may judge that $a(x)$ is altogether irrelevant (Keynes holds that this is always the case when it *merely* specifies a position in space or time). In this case the probability reduces to $\phi(x)/S(x) \,.\, b$. As a rule, however, $a(x)$ can be analysed into a number of propositional functions, and some of these may well be relevant, in which case the probabilities will in general be different.

On the other hand the probability may equally well be written

$$(x)\ \{S(x) \,.\, a(x) \supset \phi(x)\}\ /b$$

which is of type two.

In this case, therefore, the two types denote the same probability; this obviously follows from the fact that any member of a unit class and all members of the class are the same thing.

We are thus led to consider whether the two types cannot be connected in other cases. The proposal in type two is a constant, like the proposal in $\phi(a)/b$, and the arguments used in that case apply in this also. In other words it would seem that the proposition $(x) \,.\, S(x) \supset \phi(x)$ must be the subject of some description contained in b. If we could treat "y is true" as a genuine propositional function the reduction would be easy, but I am not quite sure that it is so. We should then have to write $(x) \,.\, S(x) \supset \phi(x)/b$ as

$$\frac{(y \text{ is true})}{\{y= : (x) \,.\, S(x) \supset \phi(x)\} \,.\, \psi(y) \,.\, b}$$

which expresses the fact that the probability of a generalisation depends on some property of it *as* a generalization. I do not, however, feel satisfied with this and hope to pursue the question further at some other time.

To sum up, there are, as Mr. Urmson says, two types of probability

(1) the probability that any $S(x)$ taken at random satisfies $\phi(x)$, which I have represented by '$\phi(x)/S(x) \,.\, b$'.

(2) the probability that every $S(x)$ satisfies $\phi(x)$, denoted by '$\{(x) \,.\, S(x) \supset \phi(x)\}\ /b$'.

Either of these probabilities may have a numerical value, but in practice this rarely occurs with the second.

The values of the variable in the first type are, as a rule, objects broadly speaking, but they may be propositions, and it seems likely that the second type can be brought under this heading.

APPENDIX. Proof of result on p. 74. In a large number, N, of cases the bag containing white balls only will be chosen N/2 times, and on each of these occasions the ball drawn will be white. The other bag will also be chosen N/2 times, and the ball drawn will be white on half tof hese occasions, or N/4 times. A white ball will therefore be drawn on 3N/4 occasions and on N/2 of these the bag chosen will contain white balls only. Hence the probability that, if a white ball is drawn, the bag chosen contains white balls only ; or

$$\frac{(x) \,.\, x \text{ was contained in the bag chosen} \supset x \text{ is white}}{(\text{the ball drawn was white}) \,.\, h}$$

$$= N/2 \div 3N/4 \;=\; 2/3.$$

Also, as the probability that the bag chosen contained white balls only is 2/3 the probability that it contained black and white balls in equal numbers is 1/3. The probability that the next ball drawn will be white (the first having been replaced) or

$$(x \text{ is white})$$

$$(x \text{ is a ball in the bag chosen}) \,.\, (\text{the first ball drawn was white}) \,.\, h$$

is clearly therefore $(2/3 \times 1) + (1/3 \times 1/2) = 5/6$.

This proof could also have been set out in Keynes' notation as modified in this paper, but in this case the frequency notation is adequate and is also briefer and simpler.

MORE ABOUT PROBABILITY

By C. H. WHITELEY

IN *Analysis* for October 1947, Mr. J. O. Urmson argues that the word " probable " has at least two different senses. Some statements about probability (his " type one ") are empirical statements, *e.g.* " The probability of a man of 80 living 10 more years is low " ; " The probability of suicide is higher in peace than in war ". Other statements about probability (his " type two ") are not empirical and cannot be modified by any further evidence ; *e.g.* " In the light of available data it is probable that the probability of suicide is higher in peace than in war " ; " Given that all examined cows have been ruminant, it is probable that all cows are ruminant ". I wish to examine this distinction further.

What sort of empirical fact is stated by " type one " probability statements? The most plausible answer to this question is that they state a statistical fact, a ratio between positive and negative instances. If I say " The probability of suicide is higher in peace than in war ", I am stating, or at the least implying, " Proportionately more people commit suicide in peace than in war ", which is an empirical fact. I cannot see any other empirical fact, beyond this kind of fact about the proportionate number of instances, in which the " probability " might consist. Probabilities of this kind can, as Urmson says, be given a numerical

value without change of character, because they are relations between numbers of cases. Seeing that this is so, many philosophers have straightforwardly defined " probability " in terms of ratios or " frequencies ". If such a definition is accepted, then " I see that most houses in Birmingham cost more than £2,000 ; so if I want to buy a house there I shall probably have to pay more than £2,000 for it ", would be a tautology. But it looks like an inference ; and those who speak of probability as a " relation between propositions " appear to assimilate the probability-relation to implication. Yet, if this is an inference, what is its conclusion? There seems to be no assertion appearing in the conclusion over and above the statement about the frequency of houses in Birmingham costing over £2,000 which formed the premise. It seems to me that what is reached in arguments of this kind is not a conclusion but a decision. " Probably " is a cue for action ; probability statements have the function of giving us advice. (Hence " Probably you should do so-and-so " is a normal manner of speech.) If I am interested in history or economics, I say " Very few houses in Birmingham can be bought for less than £2,000 ". But if I am concerned about changing my residence I say " I shall probably have to pay more than £2,000 if I want to buy a house ". So one talks rarely of the probability of a kind of event in general (of rain after a fall of the barometer, say) but often of the probability of a particular case (rain this afternoon), concerning which one has to take decisions and form expectations. So there is a use of the word " probable " in which its function is to recommend a course of action. What this course of action is cannot be stated precisely apart from the context. It is usually, though not always, to act as though what is said to be probable were actually the case. The probable happening is the one which we are to expect and be prepared for ; the highly improbable happening is one for which we need make no preparations. So I venture to expand " The probability of an adult Englishman understanding the word ' waistcoat ' is very high " into two components : (1) " Most adult Englishmen understand the word ' waistcoat ' ", and (2) " When we are talking to an Englishman we should assume that he will understand the word ' waistcoat ' ". The word " probability " has now disappeared. Its function in the original statement was to effect the transition from (1) to (2). It will be seen that (1) is an empirical statement and (2) is not—it is not even a statement. So, if this analysis is correct, it has disclosed, not two senses of " probable ", but two factors in the meaning of some probability-statements. One of these factors seems to be identical with Urmson's " type one ". The other resembles his " type two " at least in not being an empirical

statement. In cases of this kind, then, " An A is probably B " asserts that there are more As which are B than As which are not B, and asserts nothing else ; but it adds a recommendation that one should be prepared for an A to be B rather than for it to be not B. There is an analogy here with the analysis of some ethical statements : the difference between " X is the action which will have the best possible results in this situation " and " X is your duty " is that the latter makes a demand upon you which the former does not. And in both the ethical case and the probability case, we may ask, as Hume did, How is the transition from an empirical fact to a decision effected? In making this transition, we assume a principle which can be roughly stated, " Do not expect the unusual. If most As are B, assume that this A will be B ". And the analysis of " probability " would be incomplete if I did not point out that in reasoning about probabilities I do not merely accept and work on this principle, but I expect and require that other men will do so too, and regard it as " reasonable " to do so and " unreasonable " to do otherwise. We therefore appear to accept it as a maxim of right conduct in the treatment of hypotheses, not to assume or expect an event to happen which, if it did happen, would be very rare or unusual of its kind. This principle is not " constitutive " (as the principle of the Uniformity of Nature is), but " regulative ". It recommends a procedure, but does not assert a fact. Nor does it in any way follow from the principle of the Uniformity of Nature, being indeed a rule for dealing with classes which are not uniform. Nor does it follow from any principle of deductive logic. We find ourselves accepting it as an independent principle.

It would be generally agreed that this is *a* principle of probable reasoning ; that " Most As are B " is one kind of good reason for asserting " a given A is probably B " and for forming an expectation that a given A will turn out to be B of a degree of confidence proportionate to the proportion of As which are B. But it would not be generally agreed that it is the only principle of probable reasoning, and that all valid probable arguments make use of it. I shall maintain that this is in fact the case. As regards the probability of single cases, there is not much difficulty in seeing that my principle applies always, if we bear in mind three complications.

1. The information on which a probability-judgment is based may be either vague or precise ; more often it is vague. If I have to meet an Italian stranger at the station, I think it reasonable to look for a dark man, for I know that most Italians are dark ; but I do not know the exact proportion of blondes in the Italian population, and therefore cannot give a numerical

value to my probability. If I now ascertain this proportion, I can give the probability a numerical value. If we choose to say that this is fresh information, which provides me with a fresh probability, then it will not be the case that all probabilities have a numerical value. If we choose to say that it is not fresh information, but the same information given with greater precision, we can conclude that all probabilities, at least of this kind, have a numerical value. I do not think it matters much which way of putting the matter we adopt (though I prefer the second), so long as we recognise that wherever a vague probability-judgement is made, the possibility of a corresponding precise probability-judgement is implied ; just as, if I say that Smith is taller than Jones, I imply that he is taller by a specific number of inches, though I may not know that number.

2. When we are dealing with the probability of a particular case, we seldom treat it as an instance of only one generalisation, a member of only one class of cases. It will be a member of many classes, and thus subject to a number of different calculations of its probability. To take Keynes' example, when the barometer is high fine weather is probable; when the clouds are black, rain is probable : what is the probability of rain when the barometer is high and clouds are black? It seems to me that if we are able to estimate probabilities at all in such cases, we can still do it only by comparing numbers of instances. If the proportion of rainy days with a high barometer is smaller than the proportion of fine days with black clouds, we can decide to trust the barometer (this is all that can be meant by saying that the barometer is a more reliable indication of the weather). If we have no information of this kind, we cannot form a rational judgement. Most practical problems are more complex than this, and involve the interlocking of many probability-generalisations ; but I cannot find that probability ever rests in the end on anything other than a greater number of favourable cases.

3. Besides unusual single events, there are unusual combinations of events, *i.e.* selections from the main class. While a single Italian will probably be dark (*i.e.* most Italians are so), a collection of 1,000 Italians will probably contain a few fair individuals (*i.e.* most such collections do). The mathematics of probability is largely concerned with working out how unlikely particular combinations of cases are.

But this interpretation of the ground of probabilities seems to fail when we come to Urmson's " type two " probabilities, viz. the probability of generalisations. We say " Given that all observed cows have ruminated, probably all cows ruminate " ; and " The corpuscular theory of light is probably true ". We also say that such generalisations become more probable as

evidence accumulates. Now we cannot say that the probability of the generalisation is measured by the ratio between observed favourable cases and observed unfavourable cases. That would mean that if I have observed 5 sheep, of which 4 are white, it is 4 to 1 that the same proportion holds universally, and if I have observed 5 sheep which are all white, it is certain that all sheep are white; which is absurd. But until we have established our right to transfer to unobserved cases proportions which are known to hold in observed cases, we can make little use of any kind of probability-argument. How is such an extension justified? I believe that the probability of an inductive generalisation is established by the same principle as the probability of a particular case—viz. that the possibility of a very uncommon event is not to be taken seriously. The difference is that inductive arguments work in the opposite direction. Given that most As are not B, that we should encounter an A which is B is improbable; that we should encounter a long series of As which are all B is much more improbable, and we cannot reasonably expect to do so (in the absence of quite special reasons for such an expectation). But, conversely, given that we *have* encountered a long series of As which are all B, it is, in the same degree, improbable that most As are not B; *i.e.* to assume that most As are not B would violate our principle of not believing in highly unusual occurrences if we can help it. And if the sequence of As which are all B is sufficiently extensive, if it is a sequence which would be highly exceptional on the assumption that more than an infinitesimal proportion of As are not B (which is the case with the sequence of observed ruminant cows), then it constitutes good evidence that all As (or a majority practically indistinguishable from all) are B. Of course, an argument of this kind is only a first step in inductive reasoning; in every actual case the evidence is much more complex than "that there have been a series of As which were all B". But the mainspring of all induction is the argument from simple enumeration; and it seems to me that the force of simple enumeration must be interpreted in the above fashion.

To sum up. The function of the *word* "probable" and its equivalents is to invite us to hold provisionally a given hypothesis. The *principle* on which we proceed in making these decisions is always that of provisionally accepting the common, and rejecting the uncommon; and the more uncommon it is, the more confidently do we think that it should be rejected.